

DEPARTMENT OF APPLIED STATISTICS
UNIVERSITY COLLEGE, LONDON

Questions of the Day and of the Fray

No. IV

The Fight against Tuberculosis
and the Death-rate from
Phthisis

BY

KARL PEARSON, F.R.S.

GALTON PROFESSOR OF EUGENICS IN THE
UNIVERSITY OF LONDON

CAMBRIDGE UNIVERSITY PRESS

C. F. CLAY, MANAGER

LONDON: FETTER LANE, E.C. EDINBURGH: 100, PRINCES STREET

H. K. LEWIS, 136, GOWER STREET, LONDON, W.C.

WILLIAM WESLEY AND SON, 28, ESSEX STREET,
LONDON, W.C.

CHICAGO: UNIVERSITY OF CHICAGO PRESS

BOMBAY, CALCUTTA AND MADRAS:

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1911

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The Fight against Tuberculosis and the Death-rate from Phthisis

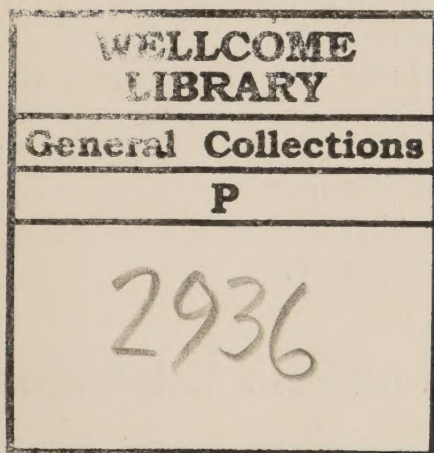
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*The Fight against Tuberculosis and the
Death-rate from Phthisis. By Karl Pearson,
F.R.S.*

The great principle of causation—*post hoc, ergo propter hoc*—discovered by those keen observers of nature, the Schoolmen of the Middle Ages, and adopted with such striking results by medical practitioners in assigning the causes of disease.—DR. CHARLES MERCIER.

IT would be quite impossible, and very unprofitable, to cite here the innumerable statements which are being made, up and down the country, by medical authorities and laymen in the course of the movement which has been named the *Fight against Tuberculosis*. The bulk of these statements are either misinterpretations of known statistical facts or are exaggerations of a character which will not bear the slightest investigation from any one trained to deal with the science of numbers. The general drift of such statements is that if a sufficient number of hundreds of thousands of pounds be forthcoming from the public purse the speakers guarantee entirely to remove tuberculosis from the list of national diseases. The line usually taken by these protagonists in the fight against tuberculosis is that tuberculosis is an essentially infectious disease, that heredity plays no part in the matter, that a great drop in the prevalence of tuberculosis has already taken place, and that this drop is due to sanitary precautions, so that if like methods be carried out on a still wider basis tuberculosis can be conquered entirely. Dr. Newsholme even tells us that in his opinion there is 'no reason why, within a relatively short period, tuberculosis should not follow the closely allied disease of leprosy towards extinction.'¹ This is to be

¹ *The Prevention of Tuberculosis*, Methuen, 1908, p. 414.

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done 'by proceeding on the tried lines' described in his work cited below. These lines are essentially what we may term sanitary methods, and are based solely on the consideration of tuberculosis as an infectious disease. It would appear that in Dr. Newsholme's opinion¹ 'the argument and conclusion that institutional segregation is the predominant cause of the decline of phthisis in this country' should be taken as the basis for administrative measures. If I do not wholly misjudge him, sanatoria are to be looked upon largely as leper houses for the segregation and isolation of the tuberculous. Thus he tells us that:

'A sanatorium, as its derivation indicates, is a place for the cure of disease, in the present connexion of tuberculosis. By Trudeau and others the word is used to denote also a hospital or asylum for hopeless cases, in which they can be cared for and treated under conditions preventing infection to others. There is some convenience in accepting this wider meaning of the term, in view of the difficulties likely to be encountered in the future² in the institutional treatment of advanced cases of the disease.'³

Dr. Newsholme having differentiated the purpose of the sanatorium into (i) a place for the cure of disease, and (ii) a place for isolation, we naturally expect some account of the former aim and its fulfilment. This, however, is confined to a single paragraph⁴ entitled: 'Medical Results of Sanatorium Treatment,' which occupies just one page of his text. And the admission here is most luminous: 'After careful consideration, I have decided not to utilize any of the many published statistics as to sanatorium treatment. So much depends on accurate diagnosis, upon accurate

¹ *The Prevention of Tuberculosis*, p. v.

² Presumably this 'future' means when phthisis shall be subject to compulsory notification, and segregation and isolation enforced, i.e. it belongs to the leper-house conception of the sanatorium.

³ *Ibid.*, p. 382.

⁴ *Ibid.*, p. 290.

tabulation of figures, and upon the lapse of a sufficiently long and uniform interval before results are tabulated, that I doubt if many of the published figures can be trusted for comparative purposes.'

What is this but the fundamental admission that in 1908 there was no reliable evidence at all to show whether sanatoria were of the least service in fulfilling their first aim, the cure of consumption? In other words, the campaign to procure funds for sanatoria which has been going on during the last two or three years is based, as far as the *cure* of tuberculosis is concerned, on no evidence as to the efficiency of sanatoria in this respect reliable enough for the chief medical officer of the Local Government Board to quote.

But it is on the sanatorium *cure* of tuberculosis, not on the value of the sanatoria as leper houses, that the public has been appealed to in this matter.¹

Failing evidence for the curative value of sanatorium treatment, we might have expected Dr. Newsholme to have said this is an unsolved problem. Not so; he immediately proceeds to recite his *credo*:

'I am completely convinced that the sanatorium treatment is most beneficial to patients, and enables a large proportion of them to resume their ordinary life. This is true even for cases in which there is consolidation, and occasionally also for cases with considerable cavitation of the lungs. Although similar cures occur apart from sanatorium treatment, clinical experience indicates that they

¹ This point is of such vital importance that it must be emphasized. At the present moment some sanatoria are building separate departments for advanced cases; others make no returns at all of advanced cases admitted to the wards, and many of them, owing to their desire to show the public a large cure rate, are refusing even moderately advanced cases. This may be effectively illustrated by a joking remark made to me quite recently: 'Owing to your criticism of the results of sanatorium treatment, we are now refusing at the — Sanatorium every patient who shows the smallest indication of tuberculosis.' In the face of such selection, how will it be possible ever to obtain a measure of the curative value of sanatoria?

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are more frequent and occur earlier under sanatorium treatment, and I have no doubt that, were exactly comparable data available, this would be found to be so.’¹

Instead of setting to work to collect the data and study them, Dr. Newsholme gives us his *belief* and appeals to those magic words which have covered so many a now-discarded process of treatment, the ‘indications of clinical experience.’ Valuable as clinical experience is and ever will be to the individual medical man, it is wholly worthless as a scientific argument until it is pooled, reduced to figures, and presented with an adequate statistical treatment.

Dr. Newsholme supports his *credo* by an appeal to an *opinion* of Professor von Ziemssen, whom for some reason he does not quote at first hand, and then by citations from Dr. J. E. Squire. The latter are so characteristic of the manner in which ‘belief’ is to take the place of knowledge that I cannot refrain from citing them in part—the italics are mine :

1. It can be ‘*reasonably expected* that of the cases of pulmonary tuberculosis which are recognized sufficiently early and commence sanatorium treatment without delay *some* may be cured and return to work in three months’.

Note this word ‘*some*,’ and even these only ‘*reasonably expected*’. We demand a comparison of that *some* against those who have been cured or who have even kept on working without any sanatorium treatment at all.

Again :

2. Three months’ treatment being *rarely sufficient* for the stage in which ‘early’ cases are generally admitted to the sanatorium, ‘we are *justified* in stating that early cases of pulmonary tuberculosis may be *expected* to recover under sanatorium treatment if persisted in *sufficiently long*,’ but six or even twelve months may be required.

¹ *The Prevention of Tuberculosis*, p. 390.

What 'justification' is there for the expectation that if three months do not suffice six or twelve may be 'expected' to?

3. The third statement of Dr. Squire's cited by Dr. Newsholme is the 'further *justifiable expectation*' that by 'sanatorium treatment even in acute and somewhat advanced cases arrest may be *anticipated* provided the patient is able to continue the treatment *sufficiently long*'.

Dr. Newsholme's book professes to be a statistical treatment—'the statistics,' he tells us, 'are indispensable to the main arguments of the book.'¹ But there is not a single figure—much less the ratio of two figures—produced to demonstrate these 'justifiable' and 'reasonable expectations'.²

Now the reader has only to proceed to the next paragraph to understand why Dr. Newsholme has passed over all demonstration of the arrest of tuberculosis as the chief function of sanatoria:

'It is not identical with the view of the public health administrator, whose question in relation to sanatoria is: By sanatorium treatment of what patients, and for these for what length of time, can I secure the greatest amount of prevention of infection?'

His answer to this occurs in the following chapter, where he tells us that the advanced cases do most harm and are most dangerous to public health. He further states that the balance of evidence is strongly against the early and moderate cases being the chief source of infection.³

In other words, the sanatoria would, from the standpoint

¹ Ibid., p. v.

² As far as the reader is concerned they amount to no more than the statement of a patent medicine vendor that as one pill produces an effect in *some* cases, if so two, we may *reasonably expect*, will when one doesn't, and if two don't, then it is justifiable to expect that more than two will. The thoughtful reader wants more than such a label, he wants the logic of an accurate statistical analysis.

³ p. 294.

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of our chief public health administrator, do most effective service if they segregated the *advanced* cases of tuberculosis. It is essentially the leper-house and not the curative idea of the sanatorium which Dr. Newsholme is emphasizing. Dr. Newsholme may or may not be entirely right on this point, but it is very clear that (i) the money collected for sanatoria is not collected on this ground, and (ii) the persons at present being admitted to most sanatoria are not the advanced cases.

It appears to me that Dr. Newsholme's views are incontestably true, provided, and provided only, we recognize infection to be the only factor in the spread of tuberculosis which needs practically to be taken into account.

But even with infectious diseases there are several points which seem to indicate that the rise and fall of the frequency of such diseases is not solely a matter of sanitary precautions and of isolation. In the actual mathematical treatment of disease frequency, it makes little difference whether the origin of the evil be transferred by the spray produced by coughing or by the bite of a mosquito. When we come to investigate, however, the manner in which such diseases spread in a community, we find that, given a definite population and a definite number of infected persons to start with, the percentage of infected persons tends either to rise or to fall, but in both cases to reach finally a certain stable value, and to maintain itself at this value. Hence in broad outline, quite independently of any sanitary measures against tuberculosis, we should anticipate that if the number of persons infected in the community with tuberculosis is at any time below a certain limit, that number will, without modification of environment, tend to decrease, and ultimately the number will become constant. This essential feature of epidemiological theory seems quite overlooked by those who, like

Dr. Newsholme, attribute *all* fall in the attack rate to improved sanitary conditions or to greater isolation.

In the case of rising incidence, as in epidemics of scarlet fever or other like diseases, a factor comes into play of acquired immunity limiting the available infectable population, and causing the attack rate to take the characteristic forms familiar to every public health officer. Few persons now would be found to attribute the fall which follows the rise of an epidemic solely, or even in great part, to the isolation or segregation of the attacked. It essentially depends on the ratio of the number of infected persons to the available infectable population ; and where this ratio is below a certain value down falls the number of infected persons till that number reaches the stability value. My reason for putting this point before the reader is not to assert that the fall in the prevalence of tuberculosis is due to the natural history of an epidemic—it may have been assisted by more extensive sanitary precautions and by greater isolation—but to indicate that an association between fall and better sanitary measures is no definite proof of causation at all, as long as we have not shown that the fall is not part of the natural history of the disease. We cannot go back with much profit—as far as the statistics of tuberculosis are concerned—in this country before 1850, certainly not before 1838. Now, on examining the data we find the death-rate from phthisis steadily increasing as we go backwards. Does any reasonable person anticipate that that increase would go on indefinitely? If so, every individual 500 years ago must have died in this country from phthisis! On the contrary, we should anticipate from our knowledge of semi-civilized people a far lower or even negligible phthisis rate. There has been almost assuredly a time in this country when the phthisis rate was *rising*, just as

through the whole of our available experience it has been falling. We have to stretch our ideas of time a little and we should realize the possibility of a typical epidemic curve in the frequency of phthisis. It is solely the logical blunder of confusing association with causation, which can attribute to everything, like sanitary improvement, which has changed continuously during the last eighty years a causal influence on phthisis.

Two chapters of Dr. Newsholme's work which seem to me to teem with fallacies arising from this confusion of association with causation are those on *Tuberculosis in Communities of varying Well-being* and *Tuberculosis in Communities with varying amounts of Institutional Segregation*.¹ The method adopted is to pick out population characters which have been more or less continuously rising or falling during the last seventy years and plot them against the continuously falling phthisis rate. Naturally there is a high correlation, and this correlation is assumed without further investigation to demonstrate a causal influence. For example, the price of wheat and total pauperism have fallen in England and the value of wages has risen, and thus correlation obviously exists between these and the phthisis death-rate! In precisely the same manner we can show that the consumption of bananas has risen continuously during the last fifty years, also the number of newspapers, and there is a very high correlation between the increased fruitarian diet or the perusal of halfpenny newspapers and the fall in the phthisis death-rate!

Again, in order to demonstrate that segregation has reduced the phthisis death-rate, Dr. Newsholme plots curves showing the continuously decreasing phthisical death-rate against the proportion of institutional deaths to total deaths.

¹ Chapter xxxii, pp. 230-51, and chapter xxxv, pp. 266-91.

Now there is a very high correlation between the national expenditure on the Navy and the proportion of institutional deaths to total deaths; are we to suppose a causal influence between the two?

Again, the insanity rate has gone up and the cancer death-rate has gone up continuously in this country; are we to suppose that the high correlation between these and the decrease of pauperism is a causal relation?

It is perfectly conceivable that increased pauperism means increased tuberculosis, for pauperism means the survival of physically and mentally inferior stocks, and family histories show that tuberculosis is frequent in such stocks, but no scientific proof whatever can be obtained by correlating two continuously changing variables whose time-changes have been in whole or in great part in one sense, and then confusing such correlation with causation.

This matter is so important, and Dr. Newsholme's method is so logically fallacious and yet so specious, that I have taken the trouble to illustrate it on two series of data I had available. Dr. Newsholme takes for example the Phthisis Death-rate in Sheffield and the Proportion per cent. of Total Deaths from Phthisis in Institutions in the same town and finds a correlation of $-.80$. He takes the price of wheat and the Phthisis Death-rates in England and Wales and finds a correlation of $+.90$. And on the basis of such high correlations he argues, *post hoc, ergo propter hoc*, that the influence of well-being and of institutional segregation have worked powerfully towards the reduction of tuberculosis. His whole argument amounts to the discovery of association followed by the assertion that it is causation. Well, I have taken the annual expenditure on the British Navy from 1871-2 to 1902-3 and correlated it with the Phthisis Death-rate in England and Wales. The correlation is $-.86$!

Clearly the increased expenditure on the Navy has been more beneficial than the increase of institutional segregation in reducing the amount of tuberculosis !

Next I have taken the influence of the 'varying well-being', not of England, but of Canada (as measured by its total exports from 1868 to 1908) upon the falling death-rate from phthisis in this country and I find it is — .82 ! For practical purposes this is again as significant as most of Dr. Newsholme's measures of the influence on phthisis of the increased well-being in this country !

Now the reader must not think this is mere trifling. It is a justifiable and apt illustration of the type of logic which abounds in Dr. Newsholme's treatise. The influence of national well-being and of institutional segregation may or may not be of first-class importance for the reduction of the phthisis death-rate. That remains to be studied. It cannot be studied in the manner adopted by Dr. Newsholme, which must sooner or later bring modern statistical methods into discredit ; not because they are really defective, but because they are handled by those who have apparently no adequate knowledge of their uses.

In nearly all the cases discussed by Dr. Newsholme *time is an essential factor of the result, time producing continuous change in one sense in both his variables, and he has made no attempt to correct his correlations for time*, i.e. to find the relations between his variables for a constant epoch.

In my opinion the chapters of his book to which I have referred are purely fallacious ; their only service can be to arouse the medical profession to the crying need for an efficient medico-statistical logic ; they have added not one iota to our knowledge of the influence of either national well-being or institutional segregation on the prevalence of tuberculosis.

In these sections as well as in other sections of his work

Dr. Newsholme, instead of admitting that the problems are unsolved, and recognizing the amount of data that is required before they can be attacked, and the subtlety of the statistical analysis which is requisite when the data are provided in order to solve these problems definitely, places before the reader a series of facts, consciously or unconsciously collected with a view to preconceived opinions, and treated by a wholly inadequate theoretical knowledge of statistics. The same remarks apply essentially to his chapters on susceptibility to infection and on lowered resistance to infection.¹ They are not only inadequate but to a large extent contradictory. If under-feeding and bad nutrition lower the individual resistance to tuberculosis, then those constitutions which cannot profitably assimilate their food must, we should imagine, be reasonably supposed to have greater susceptibility. 'If,' writes Dr. Kingston Fowler, 'of a large number of persons exposed to infection only a few acquire a disease, the susceptibility of the individual becomes a factor in causation of greater moment than exposure to infection.'

How does Dr. Newsholme meet this perfectly clear argument? He writes:

'The underlying assumption in the position taken up by those holding the view expressed in the above quotation appears to be that everybody "exposed" to infection necessarily receives an efficient dose of infection. The error of this assumption can be seen by ascertaining what happens when a given number of persons are exposed to the infection of acute infectious diseases like scarlet fever, diphtheria, and enteric fever.'²

Here we meet at once the sort of reasoning on preconceived notions that I have indicated. What evidence at present exists to show that those who escape scarlet fever,

¹ Chapter xxii, pp. 161-3, and chapter xxvi, pp. 191-3.

² Ibid., p. 162.

diphtheria, or enteric do so because they have received a lesser dose and not because they have a relatively less susceptibility? It is assumed that the 'effective dose' is the same for all persons in one set of cases, and then this mere assumption is used as an approved fact and a valid argument to reason about other cases. In neither instance does the author bring forward a single figure or cite a single reference to demonstrate the assertion! There is no danger of this passing as knowledge with the trained man of science, but when one in the official position of Dr. Newsholme lays down such dogmas they are only too liable to be accepted by the untrained public as proven truths. Until the contrary is demonstrated no one has a right to dogmatically assert that even hair or eye colour does not indicate a greater susceptibility to either attack, severity, or death in the matter of scarlet fever, diphtheria, or enteric. These are external characters which can be measured and their relationship to disease determined, but what basis can possibly exist for *a priori* declaring that the very texture of the lung tissues, the method of breathing, or the peculiarities of nasal formation have no relation to tuberculosis, or to the power of recovery after 'effective dosing'?

The facts which Dr. Newsholme has been called upon to meet and does not meet either in the book I am now discussing nor in his utterances on the subject since its publication are these:

- (i) There is a 'resemblance' between parent and offspring in the matter of tuberculosis of 0.5, and this as between father and child is as close as between mother and child.¹
- (ii) There is a much lower 'resemblance' between husband

¹ Pearson, *A First Study of the Statistics of Pulmonary Tuberculosis, Inheritance* (Dulau & Co.); and Goring, *On the Inheritance of the Diatheses of Phthisis and Insanity* (Dulau & Co.).

and wife in the matter of tuberculosis than between parent and child, say from .25 to .30, and yet the intimacy of husband and wife is in most cases a closer one.¹

(iii) The first relation is precisely that which we find for other characters where the relationship is due to heredity; the second relation is precisely that which we find for other marital resemblances, say insanity. It is therefore difficult to believe it to be mainly due to infection.

(iv) The marital resemblance varies from class to class and is least in the lowest classes of the population. Goring was unable to find any significant marital resemblance in the matter of phthisis among the very poor and destitute, although it existed to a lesser degree among the well-to-do and prosperous lower classes (.16).²

By 'degree of resemblance' is here to be understood the presence or absence of tuberculosis in both members of the compared pair.

Now these statements have been nowhere met by those who, like Dr. Newsholme, consider that phthisis depends only on effective dose and not on constitution, and yet they and he admit that fatigue, over-exertion, and injury can lower phagocytal influence,³ as if the liability to over-fatigue were not itself peculiar to certain constitutions.

Dr. Newsholme even writes thus: 'that alcoholic indulgence favours the occurrence of phthisis is shown by abundant evidence.'⁴ He does not produce this evidence in any direct way. He cites Landouzy and Brouardel's *opinions*, but he does not show that the death-rate from tuberculosis is among the alcoholic greater than among the non-alcoholic. He cites the Registrar-General's occupation statistics, which,

¹ Pope, Pearson, and Elderton, *A Second Study of the Statistics of Pulmonary Tuberculosis, Marital Infection* (Dulau & Co.).

² Goring, loc. cit., p. 27.

³ Newsholme, Chapter xxi.

⁴ Ibid., p. 181.

as will be shown on another occasion, are open to grave fallacies when used in this manner. As a matter of fact, when investigated *directly* the alcoholic appear to die of most diseases at a higher rate than the non-alcoholic, but *phthisis* and gout are the *two marked* exceptions to this rule.

The fact that this result is reached by the Imperial German Bureau of Statistics¹ is evidence that we are not dealing with data collected with any preconceived opinions, or with any personal bias. The data are provided by the Leipzig *Krankenkasse*, and the alcoholic are those upon whose sickness card the doctor had written 'P' (= *potator* or Trinker), 'Chronische Trunksucht,' 'Delirium tremens,' or 'Säuferwahnsinn'; the distinction is thus between im-moderate drinkers and the remainder. For 1,000 observed persons tuberculosis of all kinds occurred in the following proportions for three age groups:

Tuberculosis.	Ages.					
	15-34.		35-54.		55-74.	
	Alcoh.	Non-Alcoh.	Alcoh.	Non-Alcoh.	Alcoh.	Non-Alcoh.
Cases of Sickness . .	4.2	6.7	7.4	10.2	9.4	10.0
Days of Sickness . .	259	529	408	858	644	824
Deaths	1.39	1.90	1.34	3.32	4.72	3.92

The official report recognizes that with regard to tuberculosis the sickness and mortality results are more favourable to the alcoholists than to the total population. The explanation given is of course, not that alcohol protects the consumer from the tubercle bacillus, but that the men of better physique are those who take more readily to alcohol.

¹ *Krankheits- u. Sterblichkeitsverhältnisse in der Ortskrankenkasse für Leipzig u. Umgegend. Untersuchungen über den Einfluss von Geschlecht, Alter u. Beruf.* Bearbeitet im kaiserlichen Statistischen Amte. Bd. I, Teil C, S. 190-8.

An analysis of the trades followed shows this to be the case, precisely as Miss Elderton and I found for the Edinburgh data.¹ The inference then drawn is that this constitutionally greater vigour of the alcoholists protects them from tuberculosis. In other words, the German statisticians accept the view that the constitution does play a very considerable part in the susceptibility to tuberculosis. This explanation would not be accepted by Dr. Newsholme, although he is indirectly confirming the same view when he cites—what the German, the only really adequate, data definitely refute—the statement of Brouardel that ‘Alcoholism is in fact the most powerful factor in the propagation of tuberculosis’, and that of Landouzy that ‘l’alcoolisme fait le lit de la tuberculose’.

If we turn to the actual problem of the heredity of the predisposition in tuberculosis, we find Dr. Newsholme’s chapter on this all-important point perhaps the most unsatisfactory in, what I consider, a thoroughly unsatisfactory book. It is replete with sweeping statements, several of which are self-contradictory and others contradict statements in other portions of the work. Thus in Chapter xiii he tells us of circumstances limiting the amount of infection, how many consumptives are not infectious, and what a small percentage of the infective tuberculous exist in the population at any given time.²

Dr. Newsholme’s object at this stage is to minimize the opportunities each individual has of infection. But in the chapter on Heredity he cites with approval Walshe’s remark upon the fact that 26 % of his tuberculous patients came of a father or mother or both parents who were tuberculous: ‘This

¹ Elderton and Pearson: *A First Study of the Influence of Parental Alcoholism* (Dulau & Co.).

² See especially pp. 101–2.

ratio of 26 per 100 might be or probably is, no higher than that of the tuberculized portion of the population generally.' ¹ Does Dr. Newsholme really agree that 26 % of all parents have during the time they have been in contact with their children been actively infectious from the tuberculous standpoint? If he does not, why does he quote such opinions as having any bearing on the problem of heredity? If 26 % of all parents have been actively infectious, why does he fall foul of the statement I have made that in towns the artisan classes can scarcely, except by the absence of the tuberculous diathesis, hope to escape infection? He says that this statement goes beyond my data; that is perfectly true. But it does not go beyond data which were as open to him as they were to me, and which I imagined were common knowledge even to English medical students of tuberculosis. What is the meaning of such a statement as 'no point is clearer in the pathology of tuberculosis than that efficient infection depends largely on the dosage of infective material'?—Who would wish to deny such a statement? The fundamental question does not turn on this, but on whether the dosage requisite for efficient infection is the same for A and for B.

Now one of the chief things which has surprised me in reading Dr. Newsholme's book is the absence of reference to the most important of the German writings on the subject of tuberculosis. In his bibliography of 131 titles only two German memoirs are referred to directly in their German originals—a paper by Flügge and one by Heymann. This is a very small proportion—even when we allow for a few translations or abstracts from German memoirs—considering the importance and volume of German work in this field. It was not until I discovered this apparent neglect of

¹ *The Prevention of Tuberculosis*, p. 186.

German work by Dr. Newsholme that I grasped the probable explanation of his criticism of my statement that especially under urban conditions the risk of infection is so great that the constitution means everything to the individual whose life cannot be spent in self-protection. That statement was based on the facts demonstrated by German investigators that the great bulk of the population has suffered from tuberculosis, and *recovered from the disease*. There is, as far as I can discover, no evidence in the whole of Dr. Newsholme's book of more than 400 pages, any more than there is in his bibliography, that he has studied the wide German literature on the prevalence of tuberculosis among the general population. A very large percentage of the German population have at one time or another suffered from tuberculous lesions, and have recovered. There is not the least reason to doubt that a similar state of affairs exists in this country. I must run the risk of wearying the reader by citing some of this evidence, because it is vital to the question of the existence of a constitutional power of resistance to tuberculosis.

I shall cite first an author whom Dr. Newsholme could have consulted, for he writes in English, and although he is old, that is no reason for his complete neglect, namely, Henry Ancell. He wrote not only a *Treatise on Tuberculosis and the Constitutional Origin of Consumption and Scrofula*, 1852, but a number of articles on the subject, and was among the first to lay stress on the prevalence of tuberculosis as evidenced by post-mortem examination. In a paper, 'Facts and Opinions relating to Tuberculosis, with Commentaries,'¹ he cites the *Decennium Pathologicum* of Dr. L. K. Chambers, giving the results of the post-mortem examinations made in the mortuary of St. George's Hospital,

¹ *Association Medical Journal*, Nov. 25, 1853, pp. 1030-5.

in the ten years Dec. 31, 1840, to Dec. 31, 1850. The number of autopsies was 2,161. The following table exhibits the age results for male and female together:

	Birth to 15 years.	15-30	30-45	45-60	above 60	All.
Total No. of Autopsies .	154	636	651	438	167	2046
Per cent. tubercle found .	27.9	35.8	25.8	19.6	7.7	26.1

In 115 cases age or sex was not recorded and they are not entered.

It would thus appear that with the rough methods of autopsy existing in 1840-50 one quarter of all the cases that came to post-mortem showed signs of tubercle. Among the autopsies in a large general hospital those whose deaths resulted from obvious clinical tuberculosis were likely to be relatively few in number.

Oskar Müller,¹ examining 500 bodies of children from 0 to 15 years of age brought to the Pathological Institute at Munich, concluded that 150 had died of tuberculosis and 59 showed latent tuberculosis, i. e. 30 % and 11.8 % respectively.

Baumgarten, cited by Felix Wolff, found 3 out of every 4 bodies—those who died of tuberculosis not included—i. e. 75 % showed latent or healed tuberculosis.² Bollinger found 40 % to 50 % of all bodies showed traces of the disease.³ Emil Schlenker,⁴ in his paper of 1893, gives the results of others as to the prevalency of tuberculosis. Thus Babes in Budapesth found evidence of tuberculosis in about half of all the children who had died of various diseases; in 1887 out of

¹ 'Zur Kenntniss der Kindertuberculose', *Münchener medicinische Wochenschrift*, No. 50, 1889, S. 857-80 and No. 52, S. 917-20.

² Ueber Infectionsgefahr und Erkranken bei Tuberculose, *Münchener medicinische Wochenschrift*, No. 39, S. 685-9, 1892.

³ Ibid., S. 686.

⁴ Ueber die Häufigkeit tuberculöser Veränderungen in menschlichen Leichen. *Virchow's Archiv*, Bd. 134, S. 145-51. Berlin, 1893.

93 corpses he found 65 tuberculous, although only 10 to 11 were said to have died of tuberculosis. Wolff, at the *Congress für innere Medizin*, 1893, gave the following numbers: among adults 40%–50%; among children 60%–70%. About 30% of the adults and 40% of the children showed latent tuberculosis. Hanau examined 100 unselected bodies and found 66 tuberculous and 34 free from tubercle. Of 61 cases without active and evident tuberculosis, he found latent tuberculosis in 44% of them. Schlenker states that Martin, who gives 9.4%, and Fowler, who gives 9.0% of old tuberculosis amongst those who did not die of tuberculosis, cannot possibly have examined the bodies properly. Albin Burkhardt dealt in 1906 with 1,452 cases.¹ Of these 190 were children, and of these 38% were tuberculous; out of this 38%, 18% had died of tuberculosis. Of 1,262 adults, only 113, i. e. 9%, showed no tuberculosis, 91% showed tuberculosis. Burkhardt states that Dr. Schmorl in 50 private post-mortems, which it is well known must be limited to a minimum of examination, found 46% tuberculous, and in a more recent series 70%.

Before Burkhardt, Otto Nägeli² in 1900 had dealt with 500 autopsies. Of children, whom he reckoned as those under 18, he had 88, and he found only 17.0% tuberculous, but of adults, of whom he had 420, he found no less than 93.1% showed signs of tubercle. Of 111 latent inactive cases Nägeli found:

Age.	18–30	30–40	40–50	50–60	60–70	above 70
Percentage of latent inactive	33 %	42 %	53 %	58 %	56 %	55 %

¹ Ueber Häufigkeit und Ursache menschlicher Tuberkulose auf Grund von ca. 1400 Sektionen. *Zeitschrift für Hygiene und Infektionskrankheiten*, Bd. LIII, S. 139–58. Leipzig, 1900.

² Ueber Häufigkeit, Localisation und Ausheilung der Tuberkulose. *Virchow's Archiv*, Bd. 160, S. 426–72. Berlin, 1900.

Thus a very large number of persons over 40 years of age have suffered at one time or another from tuberculosis.

Hamburger and Sluka¹ have dealt with 401 cases of children between 0 and 14 years and found 40 % were tuberculous. In children from 1 to 15 years they found 55 % tuberculous. Generally they found :

	1-12 months	1-2 yrs.	3-4 yrs.	5-6 yrs.	7-10 yrs.	11-14 yrs.
Per cent. Tuberculous	15 %	42 %	59 %	60 %	64 %	77 %
Percentage of Healed	0 %	0 %	10 %	40 %	40 %	47 %

The reason for no healed cases in 2 years and under is probably to be found in the months that the healing process needs. Taking only children who have died of acute infectious diseases, injuries, or operations, these authors give the following table :

<i>Authority.</i>	<i>Town.</i>	<i>Number of Cases.</i>	<i>Percentage showing Tubercle.</i>
Councilman	Boston	220	16 %
Baginsky	Berlin	806	18 %
Heller	Kiel	714	20 %
Ganghofner	Prag	973	24 %
Hamburger and Sluka	Wien	150	21 %

In the same year as Hamburger and Sluka (1905), at the Paris International Congress on Tuberculosis Ganghofner gave figures for 1,800 autopsies on children, who during life had not been recognized as suffering from tuberculosis. He found 27 % of those from 4 to 6 and also 27 % from 6 to 8 years of age showed tubercle.

Lastly I may refer to Lubarsch,² who in 1,820 dissections found tuberculous changes in 60 % of cases, and to Comby,³ who in Paris found 67 % of children from 2 to 10 and 64 %

¹ Beitrag zur Kenntniss der Tuberkulose im Kindesalter, *Jahrbuch für Kinderheilkunde*, IV. F. 62, S. 517-36. Berlin, 1905.

² Ueber den Infektionsmodus bei der Tuberkulose. *Fortschritte der Medizin*, Jahrg. XXII, S. 669-82 und S. 701-16. Berlin, 1904.

³ *La Presse médicale*, 1906, p. 766.

of children from 10 to 16 with tuberculous lesions. This by no means exhausts the literature on the subject, but allowing for all the personal equation of post-mortem investigation, there seems little reason for doubting that at least a moiety, probably more, of the community in our large towns by the time they reach adult life have been subject to attack by the tubercle bacillus, and that the larger portion of these persons survive the attack and die from other causes. Now it is a remarkable fact that Dr. Newsholme does not cite any of the data we have quoted above, and that he does state that I go beyond my data when I state that for the artisan class the inheritance factor is far more important than the infection factor.

Thus far I have only appealed to literature which was just as open to Dr. Newsholme in 1908 as to myself. But the case for the general prevalence of tuberculosis has been made much stronger from that very date onwards by experimental work based on von Pirquet's discovery (1907) of the cutaneous reaction test.¹ This test was applied by von Pirquet himself to 693 children, and he found that for children unsuspected of tuberculosis the percentages showing by positive reaction the presence of tuberculosis were as follows:

1st Year	2nd Year	3rd and 4th Years	5th and 6th Years	7th to 10th Years	11th to 14th Years
3 %	2 %	13 %	17 %	35 %	55 %

But the cutaneous reaction test does not indicate all cases of tuberculosis. It is negative for at least some few cases of active tuberculosis, and for a considerable number of cases of apparently inactive tuberculosis, which latter, however, show according to von Pirquet (1908) a positive

¹ *Berliner klinische Wochenschrift*, No. 20, 1907.

reaction in the bulk of cases on a second cutaneous application. The active cases which do not give a positive reaction have been recently tested by a tuberculin injection. It is not my function to express any opinion either on the efficacy of the cutaneous or injectional tests or on the legitimacy of applying them to large numbers of children submitted to public care for wholly different ends. If we accept these tests, and the trend of present scientific judgement is in their favour, then the autopsy test of tuberculosis provides only a minimum frequency of the prevalence of tuberculosis—'tuberculosis may be present but has not been found' is all that can be reported in many instances. In support of the view that the injectional reaction shows cases which are not discoverable by cutaneous reaction, Hamburger and Monti state that they know up to the present, of no case of positive injectional reaction which has not shown tubercle on autopsy.¹

These same writers, believing that schools would only give them children from 7 to 14, and that they would not be granted leave to apply their tests to 'Krippen' and 'Kindergärten' so as to catch the younger children, decided to apply them to all children brought to the children's hospital in Vienna suffering from acute feverish diseases or trauma, i.e. to cases showing no connexion with tuberculosis. The children showing negative reaction to the cutaneous test were submitted to a tuberculin injection, and those 'negative' to this frequently to a second injection, which repetition, however, showed positive reaction in only two cases. Those not reacting to the tuberculin injection were considered free of tuberculosis. Of 509 children—

¹ Die Tuberkulosehäufigkeit im Kindesalter, *Münchener medizinische Wochenschrift*, Bd. lvi, S. 449-51, 1909. See also F. Hamburger, Ueber die Stichreaktion bei der Diagnose kindlicher Tuberculose, *Ibid.*, Bd. lvi, S. 22-3, 1909.

principally sent to hospital for scarlet fever and diphtheria¹—53 % showed tuberculosis, but when divided into years we have the following results :

Year of Life	1	2	3	4	5	6	7	8	9	10	11	12	13	14	Total
Number tested	23	46	56	75	50	63	46	30	35	26	29	19	17	17	(23 +) 509 ²
'Reacted'	0	4	11	24	26	32	48	22	25	22	27	18	16	16	
% Tuberculous	0	9	20	32	52	51	61	73	71	85	93	95	94	94	(51) 53 ²

According to these results, over 90 % of Viennese children after the age of 11 show signs of tuberculosis. These children were not entirely offspring of the lowest social classes but included a fair number of the lower middle class ('Kinder aus gut kleinbürgerlichen Kreisen'). The authors provide a useful table for comparison of the tuberculin tests with the frequency of tuberculosis as deduced from autopsies, which I reproduce below.

PREVALENCY OF TUBERCULOSIS AS DETERMINED BY :

Autopsy			Tuberculin Reactions			
	Lethal and Non-Lethal	Only Non-Lethal	Cutaneous			Cutaneous and Injectional
Investigator	Ghon	Ghon	v. Pirquet	Ganghofner	Hamburger and Monti	Hamburger and Monti
2nd Year of Life	40	17	2	12	9	9
3rd and 4th Years	60	30	13	27	23	27
5th and 6th Years	56	34	17	47	36	51
7th to 10th Years	63	35	35	57	47	71
11th to 14th Years	70	53	55	70	51	94

¹ The assumptions made by these authors are, of course, that scarlet fever and diphtheria show no preference for the tuberculous children, and that these diseases alone do not give a positive reaction. The general agreement of their numbers with those of von Pirquet for cutaneous reaction and Ghon for autopsy in non-lethal cases to some extent supports their assumptions.

² The authors exclude from their totals twenty-three children in the first

Now this table shows that the injectional test following on the negative cutaneous test gives a much larger percentage of tuberculous children than has been hitherto demonstrated by autopsy—either test of course indicates that a very large section of the population has been infected with tuberculosis. If we accept Hamburger and Monti's figures, since only about 15 % die in Vienna of tuberculosis we must assume that 80 % of the population who have been at one time infected remain immune to new infection. The bulk of the population has a perfectly inactive tuberculosis, which possibly is preservative rather than dangerous. The result of these investigations seems largely to justify the view that tuberculosis is a disease of childhood, resembling measles.¹ Most persons, at least in our big cities, have to go through it in infancy, and according to Hamburger 'thereby acquire relative immunity against fresh, and certainly very frequently occurring infections.' It might even be suggested that one very real danger of tuberculosis is like that of measles—a first attack in adult life. But whatever view of the nature of tuberculosis be taken we can to-day hardly question the ubiquity of the infection. Whether, however, the bulk of mankind escape lethal tuberculosis by acquired or by inherited immunity can only be ascertained by a statistical study of inheritance and environment. Husband and wife provide the requisite like environment ; parent and child the like environment plus the hereditary factor. Admitting the ubiquity of the infection, the biometricians naturally proceeded to discuss the relative importance of heredity and environment by measuring the parental and marital phthisical correlations. Against that procedure year of life none of whom showed positive reaction. They say the number was too small to calculate a percentage.

¹ Hamburger, F.: Die Tuberculose als Kinderkrankheit. *Münchener medicinische Wochenschrift*, Bd. lvi, S. 662, 1909.

Dr. Newsholme has raised the plea that we have no data to demonstrate the ubiquity of the infection!

When at least 50 %, and according to several authorities 90 %, of that portion of the adult population of the large towns which comes under the cognizance of the general hospitals show signs of having been attacked by the tubercle bacillus, I think we may safely assert two things: (i) that the infection is so widespread that the artisan class—attending as children public schools, and as adults working in factories and travelling in public conveyances—can hardly hope to escape it, and, as is shown by post-mortem data, do not escape it; and (ii) that, as the death-rate from tuberculosis is only about 8 % of the total death-rate, the bulk of the population have sufficient resistive power to survive the attack and die from other causes. It is in my opinion one of the most remarkable features of Dr. Newsholme's treatment of tuberculosis that, although he talks largely of infection, he has omitted throughout his extensive volume all reference to the whole body of literature dealing with the prevalence of non-lethal tuberculosis, and has nowhere attempted to estimate statistically the really widespread character of the infection. It is impossible to examine such data as I have cited above, chiefly from German sources, without being impressed with the importance of the constitutional factor in this matter, and although the constitution may without doubt be weakened by a wide variety of causes, our investigations in the Eugenics Laboratory show it to be, in the main and on the average of large numbers, a question of inheritance. That clinically obvious and apparent tuberculosis appears to be inherited at precisely the same rate as stature or cubit or insanity impressed me from the outset as remarkable evidence for the constitutional-immunity factor being of more importance

than the infection factor. I quite knew that as a layman I was facing strong medical opinion that infection was everything, but I failed to find in current medical work on the subject anything indicative of a logical statistical investigation of the problem, and in much that had been written I found logical blunders of the most palpable kind. I therefore persevered in my inquiry, and I believe that in the course of two years at least a healthy scepticism has come in the best medical opinion to replace a dogmatic assertion that infection is everything.¹

¹ Perhaps the most interesting evidence for this is the recent paper by William Bulloch, M.D., and M. Greenwood, Jnr., read before the *Royal Society of Medicine* this spring. They have studied the whole of the medical literature on the subject and have come to the conclusion that no effective evidence whatever has been brought forward in modern times to prove that the constitutional factor is negligible. A layman may or may not be justifiably refused a hearing, but this memoir by an eminent pathologist and a prominent member of the staff of the Lister Institute for Preventive Medicine must at least be taken into consideration by their medical colleagues.

From my standpoint a still more remarkable document is that issued by the Committee of the Welsh Memorial Scheme. It is a report on Sanatorium Treatment of Tuberculosis and bears the signatures: Clifford Allbutt, Lauder Brunton, Arthur Latham, and William Osler. It begins with a picturesque account of the battle of the pygmies and the cranes—that is of the ‘defensive forces of the body’ against the ‘tubercle bacilli’. We read, ‘What determines whether the defensive forces or the bacilli are to prove victorious? Naturally the defensive forces must be put in the best condition to fight.’ ‘If the dose of poison is a small one, the defensive forces of the body are stimulated to attack the bacilli. They go to the seat of conflict, surround the bacilli, and as the result of repeated stimulation by further small doses of poison eat up the weakened bacilli and so in time arrest the disease.’ ‘The local action of the poison also stimulates a response from the local defensive forces.’ ‘The defensive forces require a proper supply of good nourishing food.’ ‘If the dose of poison is a larger one than the defensive forces can deal with, the latter are overpowered, the bacilli are left in command,’* &c. In such language does the allegory run on! But do these distinguished medical authorities really imagine that the question of the effectiveness of sanatorium treatment can be settled by graceful allegories as to ‘defensive forces’, which appears to be a vague term used to cover, if needful, all sources of congenital or acquired

* See for the above and other extracts, *British Medical Journal*, July 15, 1911, p. 125.

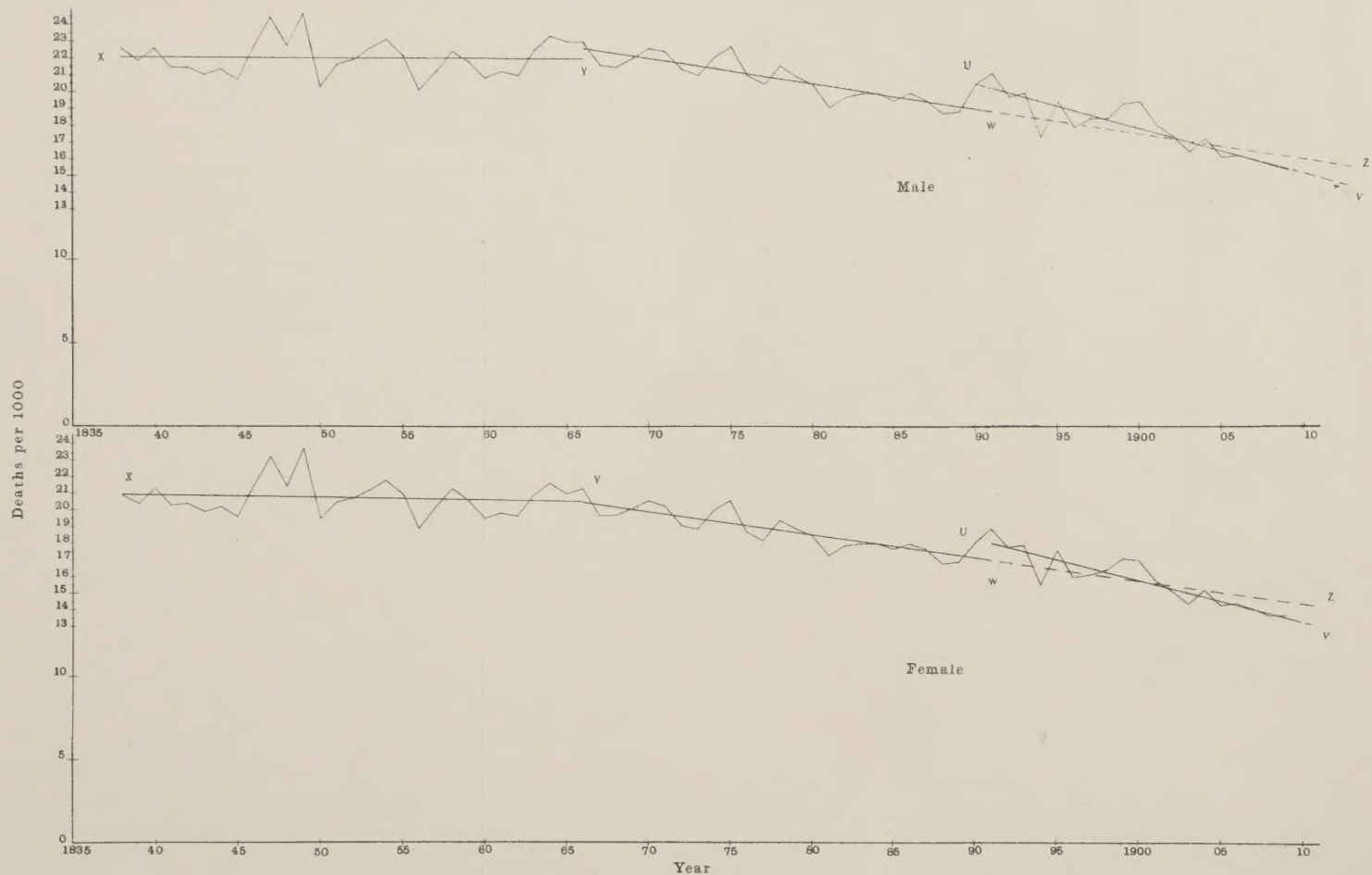


Diagram I. The Fall in the General Death Rate

Now what are the fundamental features of the decline in the phthisis death-rate in this country, features which have great weight in inducing me to believe that that decline is not very largely due to improved sanitation, still less to the modern sanatorium crusade? I put on one side the fact, which I may discuss on another occasion, that the decrease in the phthisical death-rate has gone on in districts where there has been little if any crusade. I ask the reader to look first at Diagram I, showing the fall in the general death-rate per 1,000 of England and Wales during the last seventy-five years. He will notice at once some most important features. With the kind help of Miss Ethel Elderton, 'regression straight lines' have been fitted to the graphs, in three periods: 1835-66, 1866-91, and finally 1891-1910. This last period may be said to cover modern medicine. In this period there would be greater accuracy in autopsies, and this would tend to increase the numbers of deaths accurately

immunity or resisting power, phagocytic or constitutional? Is not the real problem, that wants answering, the statistical one: To what extent is sanatorium treatment, with or without any type of tuberculin, altering the length of life of the clinically recognized tuberculous of each class? Until that problem is answered statistically what service can be done to the State, what dis-service will not eventually be done to medicine, by feeding the public with pretty allegories?

What end can be reached, what proof of the value of sanatoria obtained, by dividing the tuberculous into the four classes: Class I: Those who recover without knowing they have been infected; Class II: Those who would recover if you changed their environment; Class III: Those who need sanatorium treatment; Class IV: Those who are going to die whatever may be done to them; for such, stripped of allegorical additions, is the classification of these great authorities? The whole matter turns on the numerical proportions of these classes—a point not touched on at all in this report. The questions the public wants answered by the medical profession are simply: What have you done for Classes II and III, and how many do they represent? It is begging the whole issue to say that there are *some* patients who would recover if you changed their environment and that *others* need sanatorium treatment. What is wanted is pooled medical experience, *statistically treated*, with regard to Classes III and IV, and not the mere hierarchical expression of those 'indications of clinical experience' which, however weighty and really comforting to the individual patient, have little value in the court of science, and, as arguments for measures of great national importance, entirely fail to carry conviction.

assigned to any given disease. But since 1890 there has been little material alteration with regard to phthisis. In hospitals and medical schools the staining of tubercle bacilli was universal by 1890 and, being easy of application, would be accurate. Even inactive latent tuberculous lesions are carefully hunted for to-day. As for a lethal tuberculosis, it would be as well recognized in 1891 as in 1911. Improvement in diagnosis has probably had little effect during our last period in modifying the proportions of deaths from phthisis and deaths from other causes. Now let us look at the lines which graduate and smooth our irregular graphs for these three periods. The upper graph refers to males, the lower to females. We see that in the first period the death-rate *from all causes* remained practically stationary. Then came the second period, the period of great British prosperity and progress. In this period the general death-rate fell, and fell with extraordinary regularity. Let the reader note how nearly the line X Y is horizontal, then note the uniform steepness of the lines Y W. But the change does not end here; with 1891 we see that another change for the better has taken place. From 1891-1910 the slope of the line U V, representing best the irregular observations, has taken a quite sensible increase for both sexes. If we produce the lines Y W as W Z, it will be seen at once that the lines which adequately represent the fall of the second period, 1866-91, will no longer suffice to represent the third period. We have to admit that in the last twenty years, during which the knowledge of the tubercle bacillus and of the infectious character of tuberculosis has been general, there has been an accelerated fall in the *general* death-rate of both men and women. We have entered a period in which the *general* death-rate has been falling very sensibly beyond what happened in the previous thirty years of sanitation.

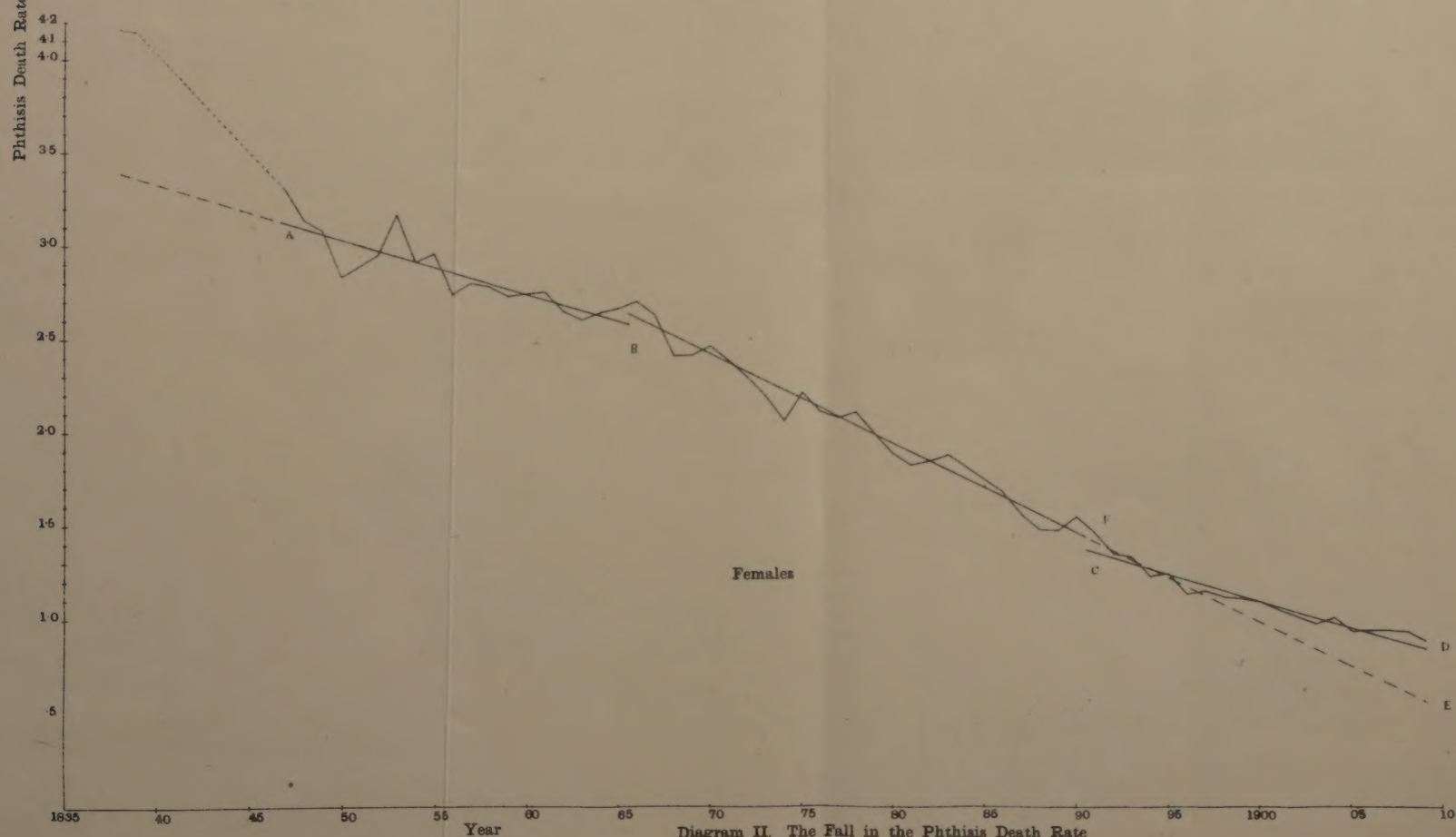
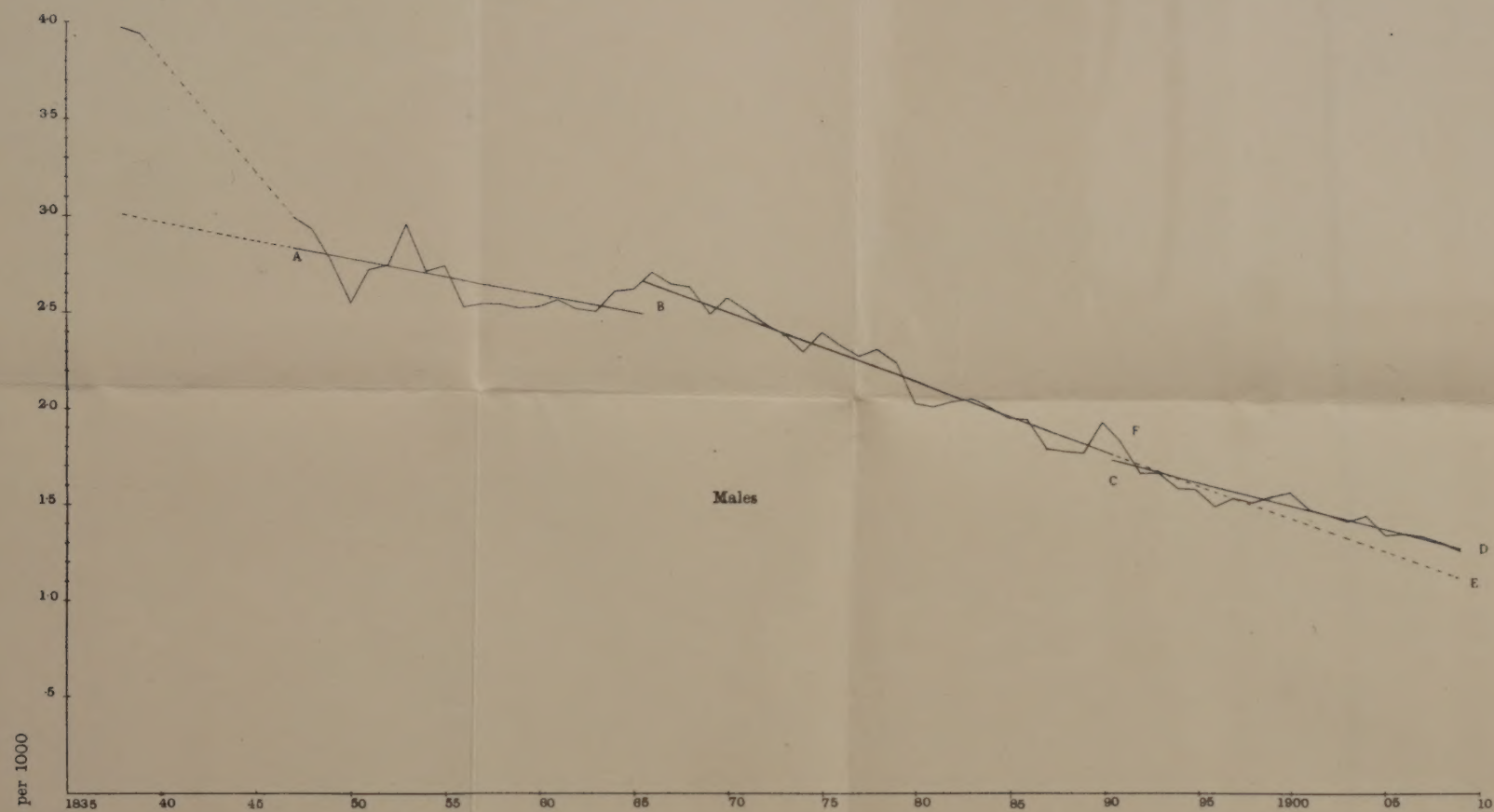


Diagram II. The Fall in the Phthisis Death Rate

Now let the reader turn to Diagram II. This represents the male and female death-rates from phthisis from 1847 to 1910. I have to thank the Registrar-General for taking out especially for me the figures for the earlier years; I was not able to use the partial results for the earlier years 1838-42, while those for 1843-46 were entirely wanting.¹ Again it was after some investigation seen that the best representation by straight lines would arise by dealing with three periods, the first up to 1866, the second 1866-94, and the third from 1894 to 1910. These periods coincide approximately with those we have considered in the case of the general death-rate. But at once what a marked difference is visible in the results!

In the first period (where the line A B has been fitted from the better returns for the years 1851-66 only) we see for both sexes a marked decrease in the phthisis death-rate. In other words, during the years preceding those in which there was an appreciable fall in the general death-rate—a fall usually attributed to the increased sanitation and better public health administration of the period 1866-94—the phthisis death-rate was falling almost as quickly as during the period of marked advances in public health administration. During this, the second period, the fall in the death-rate was sensibly accelerated, as shown by comparing the slope of the lines A B with that of B C. But when we come to the third period, the period of fifteen years during which we have known all about the tubercle bacillus, during which the current medical opinion has been that the solely important factor in tuberculosis was infection, and

¹ So far as the results provided go, they seem to indicate that the fall in the phthisis death-rate was immense between 1838 and 1847, a period of less sanitation than the following years.

during which the battle against consumption has been carried on by out-door treatment, by tuberculin, and by sanatoria, what do we find? Why, that the change in both male and female death-rates from phthisis, unlike that of the general death-rate, has ceased to be accelerated, it has ceased to fall at the steady rate of the second or improved sanitation period, it shows signs of becoming stationary, or the line C D, which represents the fall in the death-rate during this period, has begun to rise above F E, the line of normal fall from 1866-94. That is: *the death-rate from phthisis during our period of supposed greatest knowledge and during the time of our most active campaign against tuberculosis has ceased to be reduced at the old rate. There is actually a slackening in the rate of fall.* Increased knowledge and increased medical activity have not been associated with that acceleration in the fall of the phthisical death-rate which we should have anticipated.

It is of course true that the accuracy of diagnosis has altered much in the course of the years under discussion—but this tells both ways, and it can hardly be asserted that owing to any better recognition of lethal tuberculosis there has been an increasing number of deaths assigned to tuberculosis in the last fifteen years, which has swamped a further real fall in the phthisis death-rate due to increased segregation and improved treatment. The salient features of the case will hardly be modified largely by any hypothesis of better diagnosis. These features are: that the phthisical death-rate was falling rapidly before 1866, when the general death-rate for the first time began to fall so remarkably, and further that it has ceased to fall so rapidly in the third period—that of increased medical knowledge and marked change of treatment—although the fall of the general death-rate has been still further accelerated. To illustrate these points still more emphatically and from another standpoint

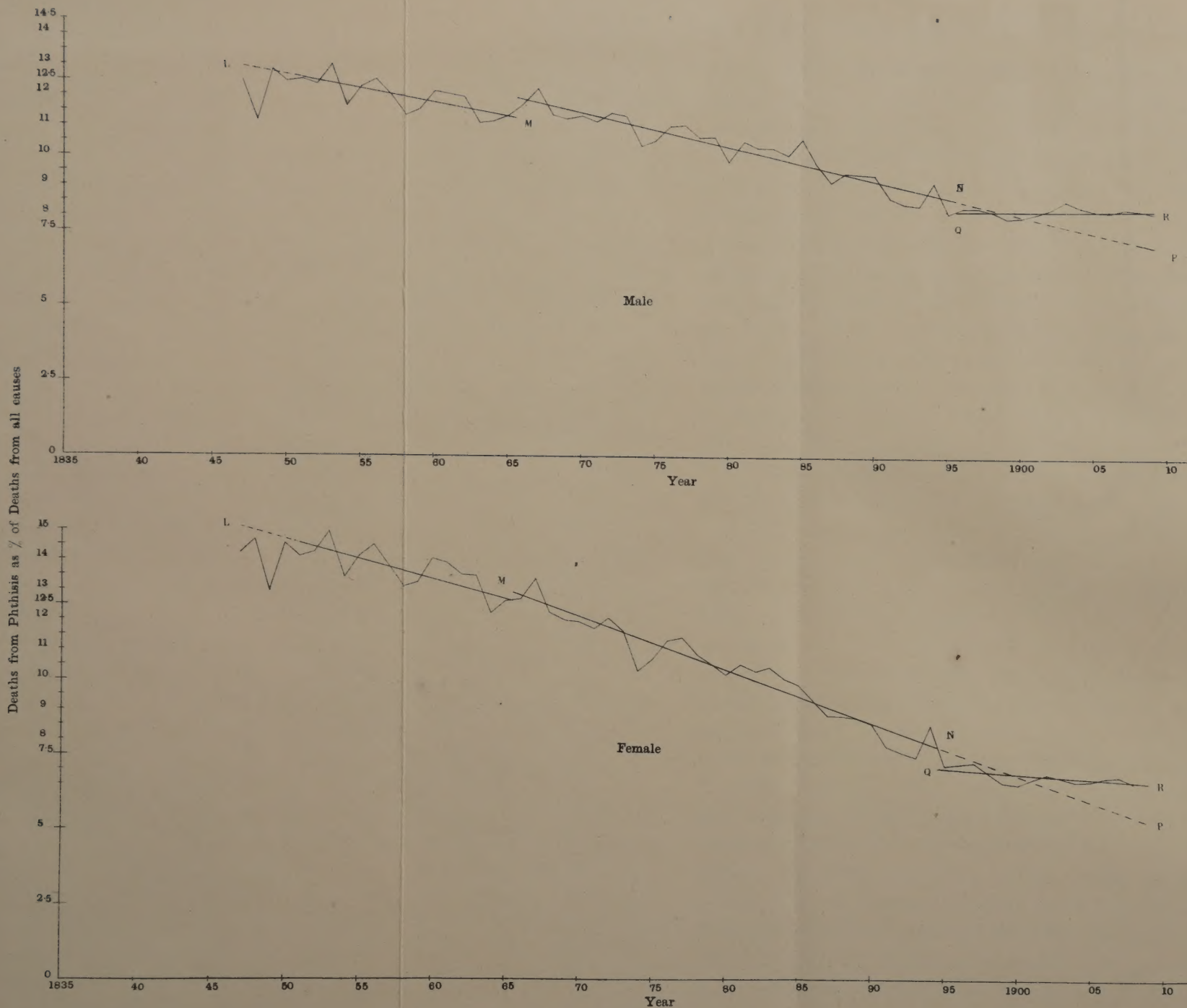


Diagram III. The Fall in the Percentage of Phthisis Deaths and the check to the Fall since the Tubercle Bacillus has been discovered

Diagram III has been prepared. This gives the percentage which phthisical deaths have been of all deaths from year to year for the last sixty-six years. The results are so doubtful before 1847 that they have not been taken into consideration. We see that the deaths from phthisis have fallen in the case of males from about 13 to 8 and for females from about 15 to 6 per cent. of all deaths. But in our first period, 1847-66, during which the general death-rate remained stationary, we see that the line L M (calculated on the basis of the fifteen years 1851-66 as much more reliable than the earlier returns) has a very significant slope; phthisis had begun to decline relative to the general death-rate before the onset of the second period with its great advances in public health administration and in general sanitation. During that second period the line M N shows us that the fall in the phthisis death-rate was slightly more accelerated than the general death-rate. But when we come to the third period, the last fifteen years during which we have known all about the tubercle bacillus, we find that the percentage of deaths from phthisis has become absolutely stationary, or rather we notice that for males it has been slightly *rising*, while for females the very slight apparent fall is really a rise, if we limit ourselves to the last ten years.¹ In other words, the *relative* number of deaths from phthisis has not been diminished by the campaign against the tubercle bacillus, although the absolute number has fallen, but at a sensibly retarded rate.

To my eye the graphs on Diagram II seem to indicate that the phthisis death-rate is tending to 'asymptote' to a line parallel to the horizontal axis of years. The curve does

¹ The lines Q R will be seen to be horizontal or nearly so, while the old relative rate of reduction during the sanitation period 1866-95 would, if maintained, have followed N P, which falls markedly below Q R.

not show a tendency to cut the horizontal axis—to reach that abolition of phthisis of which we have heard so much recently—but to become parallel to it. If the fall in the phthisis death-rate had been solely or even largely due to the so-called battle against consumption we should have anticipated that with the increased activity of the last fifteen years—the introduction of open-air treatment, the large increase of sanatoria, and the widespread use of tuberculin—the fall in the phthisis death-rate would have been accelerated; instead we have to record for these fifteen years a sensible retardation in the fall.

It is extremely difficult to reconcile the fall in the phthisis death-rate before 1866 and the retardation in the fall since 1895 with any hypothesis that the fall is the result of increased segregation of the tuberculous, of bettered environment, or of any change of treatment. He would be a rash representative of the faculty—and yet I have come across such—who could directly point to these diagrams and say that fall is the result of the labours of the medical profession and the public health officer. He would have to explain why the phthisical death-rate fell long before the general death-rate, and why the intensified campaign against tuberculosis has been accompanied not by a quickened but by a retarded rate of fall, while the general death-rate itself has for the same period a quickened fall.

Why does any infectious disease show a rising and then a falling incidence? Why do the number of cases tend to reach a stable and stationary value? Surely the key-note to the position depends on the ratio of the number of non-immune individuals to the population at large. Has or has not the selection due to many years of heavy phthisis mortality left us with a more immune and resistant population? If it has—and that I believe will be found to

be the ultimate explanation of the fall, especially the *retarded* fall in the phthisis death-rate—then infection is not the only factor worth investigating. There is the question of hereditary immunity. It may be a bitter pill for mankind to swallow, when we suggest that natural selection may have done more for racial health in this matter than medical science, but it may have its compensations from the economic standpoint. Above all, it may suggest that Evolution helps man better than he at present knows how to help himself, and that possibly he would learn to help himself better if he studied her processes of racial selection a little more closely.

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